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SOME THOUGHTS ON THE LAST QUARTER CENTURY IN PSYCHOLOGY.

IN the editorial preface to the first volume of this REVIEW, the following words occur: "Psychology, which by enlarging its field of observation and improving its methods of investigation, has within the last decade probably outstripped every other province of human knowledge in the rate of its growth." There are many who, like the writer of the present article, whose own acquaintance with psychology is exactly twenty-five years old, can vividly recall the irresistible attraction with which this rapid flowering of a new science drew them to work in its soil, and the enthusiasm with which they entered a field that promised so rich returns. It is interesting to look back, as one reaches the end of a quarter century, and ask whether one has reason to be disappointed in the continued progress of the science. It should not, indeed, have been expected to continue to outstrip all other provinces of human knowledge; we should be contented if it has maintained a steady and healthy growth. For while the fact that one discovery gives rise to many others would appear to make possible the advance of a science in geometrical ratio, this rate of progression is modified by the fact that difficulties also have a fashion of increasing geometrically. Within the scope of a short article it would be absurd to attempt an account of the accomplishment of psychology in the last twenty-five years. I shall try only to set down a few reflections suggested by the retrospect.

"Enlarging its field of observation"—has psychology continued to enlarge its field of observation? The accumulation of new data within its original field is testified to by the fact, among others, that the fourth edition of Wundt's *Physiologische Psychologie* has 1,248 pages and the sixth edition 2,185 pages. The two editions are twenty years apart in date, and the plan of the work has never been substantially altered. The advance into new regions is suggested by a comparative study of the first volume of *The Psychological Index*, and the last volume, twenty years younger. The following are samples of many headings used in the last volume and unknown to the first volume: Social Psychology, Educational Psychology, Psychoanalysis, Psychology of Values, Psychology of Testimony, Religion and Myths, Advertising. When THE PHILOSOPHICAL REVIEW was founded, there were in existence two American psychological journals, both established and edited by G. Stanley Hall: *The American Journal of Psychology*, five years old, and *The Pedagogical Seminary*, one year old. We now have in America, exclusive of monograph series, special journals devoted to non-experimental articles, to digests of psychological literature, to abnormal psychology, to mental defectives, to religious psychology, to race psychology, to educational psychology, to animal behavior, to psychoanalysis, to experimental results, and to applied psychology so far as it is not covered by other journals. Certainly, it does not look as though psychology had ceased to widen the borders of its domain.

"Improving its methods of observation"—this, after all, is the real touchstone of progress. It is of little credit to a science to push its endeavors into new fields if the exploration is carried on without proper equipment. When students of other sciences read popular accounts of the wonders of applied psychology, for instance, we cannot blame them if they say that all psychologists are either liars, or ignorant of the first principles of scientific method. James,[1] in the second number of this REVIEW, replying to Ladd's criticism of his conception of psychology, said, "We live surrounded by an enormous body of persons who are most definitely interested in the control of states of mind, and incessantly craving for a sort of psychological science which will teach them how to act. What every educator, every jail-warden, every doctor, every clergyman, every asylum superintendent asks of psychology is practical rules." This pressure from the practical side, far greater than has been experienced by any other science, is responsible for an overestimation on the part of the public and a few ill-advised psychologists, of the present equipment of applied psychology. The fact is, of course, that we have as yet no really satisfactory tests, for instance, of the fitness of an individual for a given vocation. Both in this field and in psychological investigations of the technique of advertising, the most formidable difficulty is that of reproducing in the laboratory the emotional conditions of actual life. What a man will do in a laboratory test is a very imperfect indicator of what he will do under the stimulus of the great vital motives in an actual vocation. His response to an advertisement in the laboratory may be quite unlike his reaction when there is really a question of spending his hard-earned money for an advertised article and doing without something else. Again, experimental tests of the credibility of witnesses are perfectly feasible and valuable, with the rather difficult proviso that the witness is willing to coöperate with the tester. But in spite of these discouragements, nothing offers more hope for the future of experimental applied psychology than the personnel of its present leadership. This year there is being founded "The Economic Psychology Association," with an executive board composed of the heads of great manufacturing concerns, the general superintendent of the Pennsylvania Railroad, representatives of life insurance companies, and four well-known psychologists. The Carnegie Institute of Pittsburgh is calling to its aid a strong staff of psychologists to help solve its eminently practical problems. It is impossible to associate such names as Thorndike, Hollingworth, and Scott, to mention no others, with unsound scientific methods, and whatever reproach has attached to overconfidence in the field of applied psychology will soon be removed.

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[1] "A Plea for Psychology as a 'Natural Science.'" PHILOSOPHICAL REVIEW, Vol. 1, p. 146.

The formulas for calculating correlation, which psychology has received from the statistician, are perhaps the most useful of the new tools which the science has gained during the quarter century. Not only do they throw much needed light on the real significance of mental tests, but they promise great service to general psychology. A test, of course, has to be a procedure whose results can be expressed in numerical form. This requirement can be easily met where the function tested is simply the ability to discriminate between stimuli; we need only secure physical measurements of the stimuli. But for testing such higher mental processes as attention, recall, recognition, the construction of new ideas, we are restricted to measuring either the amount of work accomplished in a given time or the amount of time required to accomplish a given task. And neither the time required for mental work nor its external results, in the form of the number of syllables learned, of additions made, of letters cancelled, for instance, give us adequate information as to the precise mental functions through the exercise of which the work was done. Thus it is often said of the mental tester that he does not know half the time what he is testing. But if we can arrange the various individuals of a large group in the order of their excellence of performance in two tests which are of seemingly disparate function and find by the use of a correlation formula that excellence in one involves excellence in the other, then we know that the functions tested have a common factor; if on the other hand the results of testing what is supposed to be the same function by two different methods give a low correlation, we know that unlike functions were really involved.

Tests of normal mental capacity are still undergoing such analysis by the correlation method, and are therefore in a formative period. Defective mental capacity offers a much simpler problem, and by using the latest revision of the Binet tests, supplemented by a large and increasing number of other tests, psychology has been enabled to enter on a work of the highest practical importance in separating mentally defective from normal children. It is able also, in the case of certain elementary processes of education, to measure with a very fair degree of accuracy the efficiency of teaching, as by the use of the Courtis arithmetic tests.

Social psychology is one of the new fields as yet almost untouched by experiment, with the exception of the experimental study of suggestibility inaugurated by Binet. Comparative psychology, on the other hand, has the most brilliant record of any new field as regards the development of methods. The technique of studies on the sensory discrimination of animals has wonderfully advanced since Graber and Lubbock attempted to base conclusions as to an animal's power to discriminate stimulus differences on the stimulus preferences it displays. We now supply carefully graded motives in the form of electric shocks; we present the stimulus in the purest form by the use of elaborate apparatus; we guard against giving the animal extraneous cues by keeping carefully out of sight and hearing; we can, if we choose, measure the fineness of the discrimination by the salivary reflex method, and if we are sure the animal will not be made abnormal in other ways, we can settle the question of the dependence of a given discrimination on a certain sensory apparatus by extirpating the apparatus. The scientific, experimental study of animal learning has been created in the past twenty-five years. The possibility of 'free ideas,' or their functional equivalents, in animals, the most difficult problem of comparative psychology, clearly formulated by the work of Thorndike, has been illuminated by Haggerty's demonstration of the occurrence of rational imitation in monkeys, by Hunter's method of delayed reaction, where an animal is required to wait a certain time after a stimulus has ceased before responding to it, and by the multiple choice method of Hamilton and Yerkes. These methods, with the conditioned reflex method in the hands of Watson and his pupils, offer chances for a direct comparison between animal and human learning.

So much for methods in the new fields of psychology; what of the status of methods in the old field of general psychology? The contest is warm at the present time between the adherents of introspection and its enemies. Introspection had its opponents twenty-five years ago and more, but the direction of the attack upon it has somewhat shifted. At the outset the objection was that introspection was in principle impossible; that states of consciousness could not themselves be the objects of examination by states of consciousness; that they must inevitably dissolve or be seriously distorted in the process. Now the line of attack is that introspection has produced no results of scientific value. There seem to be three different positions from which this attack is delivered. The most extreme is the view that inner experience, with which introspection deals, is wholly unimportant and uninteresting, if indeed it has any real existence at all. The non-existence of all inner experience, which is the logical deduction from some statements of the 'behaviorists,' of course really implies the non-existence of all experience whatever, and need not be given serious consideration. Titchener, in an article published a few years ago,[1] reports that at a meeting of experimentalists, it was cited as evidence against introspection that in tachistoscopic experiments it gives a wholly distorted account of the stimuli presented. Titchener urges that introspection does not undertake to describe stimuli, but conscious experience. What was actually, however, at the root of the criticism in the objectors' minds was probably the fact that they were not really interested in states of consciousness, and therefore thought an account of them worthless. It is sufficient to say in criticism of this attitude that it assumes the personal interest of the individual who maintains it, as the standard of what is worth while for science at large.

The second hostile position to introspection argues that where introspection is the only method of investigating a phenomenon, its results are so self-contradictory as to be worthless. (In distinguishing this position from the first and from the third, yet to be mentioned, I do not mean of course to imply that the same writers do not hold all three positions). Thus, "I firmly believe," says Watson in his first proclamation of his behavioristic creed, "that two hundred years from now, unless the introspective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color; whether there is a difference in 'texture' between image and sensation and upon many others of like character."[2] There is no doubt that introspection is vulnerable at this point; careful, expert introspective work on the thought processes, for example, where no objective methods were practicable, has led to diametrically opposed conclusions with regard to the existence of 'imageless' thought. But let us postpone further consideration of this regrettable state of affairs until we have discussed the third accusation against introspection, namely, that it is valueless even when used in combination with objective methods for the investigation of mental phenomena.

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[1] "The Schema of Introspection," *American Journal of Psychology*, Vol. xxiii, p. 489.

[2] "Psychology as the Behaviorist Views It," *Psych. Rev.*, Vol. xx, p. 164.

What, as a matter of fact, have the results obtained by objective methods during the past quarter of a century owed to introspection? They have owed to it, one may say without extravagance, by far the greater part of their significance. In the early days of experimental psychology, many of its workers, delighted with the idea of psychology as wholly comparable with the physical sciences, proceeded with psychophysical experiments as if all that were necessary were to apply a measurable stimulus and take out a measurable reaction; as if the human organism were as simple as a balance whose swinging always indicates the same thing, a certain amount of weight applied. The possibility that two reactions equal and identical, so far as physical measurements could show, might represent entirely different events intervening between stimulus and response, was altogether neglected. Each of the three chief topics of psychophysical investigation, sensory discrimination, reaction time, and 'time sense,' was destined to have its early results wholly transformed and reinterpreted by the later workers who realized that unless introspection could throw light on the inner processes between stimulus and reaction, the numerical results were meaningless. Meumann's work on the time sense superseded that of all his predecessors by virtue of his use of introspection.

The distinction between sensorial and muscular reactions was of course due to introspection, and Ach more than doubled the value of the reaction experiment to psychology by making it an instrument, through the purely introspective controls he demanded of his observers, for investigating the strength of both associative dispositions and 'the will.' It was only through introspection that we discovered the fallacy of the assumption that when the observer is required to react after discriminating one stimulus from another, the time of the act of discrimination can be found by subtracting ordinary reaction time from the total time. In the "Unterschiedsempfindlichkeit" of Martin and Müller we have a witness to the importance of applying introspection to the processes of sensory discrimination. The most recent works on psychophysics, Wirth's *Psychophysik* and Gemelli's *Il Metodo degli Equivalenti*, recognize the essential necessity of introspection: In the former the effect of the mental attitude of the observer is emphasized at the outset, while the latter explains his results in the realm of tactual space by referring to the influence of visual imagery, a suggestion first made by the author of the present article.

What has been the value of introspection to the objective methods used in the study of the learning processes? It may be indicated by a single fact: The greatest living worker in this field, G. E. Müller, the developer and standardizer of nearly all the objective methods for the study of memory, is also the author of the most thorough and sympathetic study of the method of introspection, of the best ways to avoid its dangers and safeguard its excellences. Is it likely that so fruitful a producer of objective results would be unreasonably biased in favor of an unscientific subjective method? Let us note only a few of the established results in the field of memory which are due to introspection. One can name them almost at random: the whole matter of the use of various types of mental aids in memorizing; the reason why the amount of labor expended in memorizing increases less rapidly than the amount to be learned; the effect of learning with intent to reproduce after different periods of time; the different learning types; the reason why the 'whole method' of learning is less valuable for children than for adults; the reason why the first repetition is more valuable than later ones.

Take, finally, that field where introspection would seem to a superficial consideration almost fruitless, abnormal psychology. From the point of view of the most recent theories in this realm, that which appears in consciousness and is thus open to introspection is misleading, and needs interpreting by reference to tendencies and 'mechanisms' which work below the level of consciousness. Yet it would not be extreme to say that every-thing of value in the conceptions of the modern psychoanalytic school rests on introspection. Upon what kind of evidence is the assumption of such mechanisms as compensation or rationalization supported, save our introspective testimony that we sometimes catch ourselves in the act of whistling to keep up our courage or assigning to ourselves false motives for our conduct? On the basis of this introspective evidence we can believe that these processes occur in us even when we are not introspectively aware of them, but if no one had ever been aware of them in himself, they never would have been discovered at all. How could it ever have been ascertained that lengthened or otherwise abnormal responses in free association experiments are 'complex indicators,' if the introspection of certain observers had not borne witness to the existence in their minds of emotional disturbances accompanying such phenomena? Given this evidence, we can infer the presence of emotional disturbance not accessible to introspection from the occurrence of complex indicators, but only evidence originally derived from introspection can support such an inference. Psychoanalysis itself is of course nothing but a very rough and unscientific form of introspection. The mechanism of symbolism, to many of us the most untrustworthy assumption in the psychology of Freud and Jung, is dubious just because introspection gives uncertain testimony to it. Any two ideas in the universe may be so connected by associative links that one could serve as the symbol of the other, and if it be asserted that these links need not be accessible to introspection, we have absolutely no means of proving or disproving that a given idea did or did not act as the symbol of another given idea. The new abnormal psychology has obtained its hold, not by the presentation of objective results, such as statistics of cures, but by its appeal to the witness of introspection. When the ravings of a maniac were held to have no relation to mental facts, they could be explained only as due to physiological processes not to be introspectively studied; the new methods would connect them with mental causes not indeed open to the maniac's introspection, but similar to experiences accessible to the introspection of minds in a more normal state.

When we consider, then, the supreme importance of introspection as a supplement to objective methods, we need feel no discouragement in its defense. We postponed considering the less hopeful status of introspection in fields where no objective methods are available, as for instance, that of the higher thought processes. I am inclined to think that the unsatisfactory condition of affairs here is our own fault. Why should we not recognize that conflicting descriptions of the same experience, on the part of trained introspectors, are each of equal value and authority, and simply mean that the experience in question really differs in different minds? Why not, instead of arguing that 'imageless thoughts,' for instance, mean that their possessor does not know how to introspect, and that if he did he would be able to identify their sensation components, admit that real individual differences between minds may be the basis of these differences of opinion? The differences of opinion would then, instead of delaying the progress of science, serve as valuable scientific material. Let us not claim supreme authority for our own introspection and deny the authority of that of others. Let us, in general, welcome every new method without prejudice; let us in repeating the work of others rejoice more when we confirm it than when we overthrow it, even though our personal reputation may profit more in the latter case; let us console ourselves when our own work is overthrown by the thought that at least we may serve our science in the capacity of warnings against a false road. Thus the present paper, which began with a text, ends fitly with an exhortation.

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